plate, the coxa, the first joint being in fact the coxa and the trochanter, the short joint articulating it to the femur. Then follow short sections on the metamorphoses of insects and their habits and haunts, and longer sections on the collecting and preserving of perfect insects and larvæ, which are far more correct than the preceding ones, though very slight and quite insufficient for the initiation of a beginner. The main part of the book is devoted to short descriptions of the more prominent British insects under their various orders and families, and illustrated by twelve coloured plates, which are decidedly good for cheap chromographic work; this is by far the most useful portion of the book, and well-marked forms will easily be recognized from the figures and descriptions, even though many species are placed in their wrong families.

Ostwald's Klassiker der Exakten Wissenschaften. Nos. 31-37. (Leipzig: Wilhelm Engelmann, 1892.)

We have already called attention to this admirable series of small volumes. It consists of scientific papers which may be said to have marked definite stages in the development of science. The only fault we have to find with the series, as we have already stated, is that only the German papers are given in the language in which they were originally written. All the others are translated. This is undoubtedly a mistake, for much may often depend on the precise words used by a great master of research. In other respects the series is excellent, and should be of genuine service to scientific students. The papers reproduced in the present set of volumes are Lambert's "Photometrie," three volumes (1760); photo-chemical researches, by R. Bunsen and H. E. Roscoe (1855-59); an attempt to find the definite and simple conditions in accordance with which the constituent parts of inorganic nature are connected with one another, by Jacob Berzelius (1811-12); on a general principle of the mathematical theory of induced electrical currents, by Franz Neumann (1847); observations on the moving power of fire and the machines fitted for the development of this force, by S. Carnot (1824).

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended or this for any other part of* Nature. *No notice is taken of anonymous communications.*]

"Aminol, a True Disinfectant."

Will you grant me space, in order to avoid misunderstanding, to make the following explanation?—

(1) I recently learned that certain samples marked "Aminol, a true disinfectant" have been sent to various gentlemen, accompanied by a leaflet, in which my name, without my authority, is associated with those samples. Allow me to inform your readers that those samples contain "aminol" in water in the strength of one in five thousand. Now, the experiments which I carried out with "aminol," as regards its disinfecting power of microbes, were made with a solution of the strength of one in six hundred, and the disinfecting power of this strength was the following: spores of *Anthrax bacilli* remained unaffected after eight hours' exposure, only after an exposure for twenty-four hours did the number of living spores decrease, but some escaped disinfection even after so long an exposure. *Anthrax bacilli*, *Staphylococcus aureus*, and others were destroyed, but only after a prolonged exposure.

(2) A substance is advertised and circulated under the name of "Periodate crystals," and is associated with my name without my authority. Until quite recently I have made no experiments with it. A few years ago I made a few experiments, merely of a tentative character, with a solution which was labelled "Periodate," but not with the substance advertised as "Periodate crystals." With these latter I have recently made experiments, and I find that their solution in full strength has no disinfecting power on microbes, pathogenic and non-pathogenic, amongst which may be mentioned the bacillus and spores of anthrax, the bacillus of typhoid and of diphtheria, of cholera and of erysipelas, the *Bacillus prodigiosus*, the *Staphylococcus aureus*, and others. Likewise I find that injection of large quantities of the solution into guinea-pigs already infected with anthrax or diphtheria, has no power whatever in arresting or altering the normal course of these diseases to their fatal issue.

E. Klein.

Tracery Imitation.

I took occasion some months ago to publish the result of observations on my child H.'s progressive attempts at drawing after outline "copies" set before her.[1] Examination of the series of her drawings made almost daily during the period from her nineteenth to her twenty-seventh month showed in them no apparent form or shape. They are simply vigorous pencil markings, answering as well to one "copy" as to another—or to none. Quite suddenly, however, in her twenty-eighth month, she seemed to catch the idea of breaking the "copy" (man) up into parts, and succeeded in making head, body, arms, legs, &c., in sufficient degree of relative proportion to show that here was, in her case, the rise of what I called in the article cited, "tracery imitation" of a visual picture.

At that time I had no explanation to offer, but simply recorded the observation. I have now, however, reached a way of explaining the rise of this apparently abrupt connection between muscle-sense and sight—an explanation suggested to me by a passage in Stricker's argument for the eye-movement theory of the visual apprehension of figure or outline.[2]

Before a child begins to acquire "tracery imitation," his drawings have no shape, but they show uniformly certain systems of angles, curves, &c., due to the easiest and most natural movements of the arm. The eyes, however, have been in a measure already educated to recognize certain shapes or "copies." There are, therefore, in consciousness two series of associations—one of eye-movement sensations, a, a^1, a^2, a^3, a^4, &c., with a certain strength of revival, which we may call x: the other, an associated series of arm-movement sensations, n, n^1, n^2, n^3, n^4, &c., representing a path of least resistance in arm movement. Let us call its strength or degree of tendency to progressive revival y.

Now, before the rise of "tracery imitation" y is greater than x, for the reason that the arm is restricted to a very few movements, and these are largely automatic. Once start one of these movements, and the tendency to carry it out is very strong. The tendency of the eye-movement series, on the contrary, to regular revival is slight; very few objects, copies, &c., being so clear and isolated as to give frequent unbroken reproductions. Consequently, the arm-movement series, n, n^1, n^2, &c., wins the day, and an abortive "drawing" is the result.

But the time comes soon when the reverse is true—when x is greater than y. The eye-movement series gets strengthened constantly by the repeated exploration of familiar figures, especially if, as in the case of my child, the eye be trained by having the same "copies" set from day to day. On the other hand, the arm and hand movement series gets constantly lesser and weaker, since the increasing mobility of the muscles, in the varied new activities of this period of infancy, is acquired at the direct expense of the early "cast-iron" reactions which are largely organic. Both of these tendencies were very marked in H.—the first, in the more pronounced recognition of the "copies" set before her; the second, in the less uncouth manner of holding her pencil, moving the fingers, disposing the arm, &c. Hence, it is simply a matter of education that x should soon overweigh y, and the elements of the eye series a, a^1, a^2, a^3, &c., should draw after them the arm series.

An association thus begins to be formed between the several members of the a series and certain correct elements of arm sensation: these latter go to form, under this leading, a new n series, which gradually becomes independent as an acquisition. That each new tracery combination is thus learned separately is seen in the fact that after H. learned to trace certain "copies" (man, bird), she was yet entirely unable to trace any others.

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[1] *Science*, New York, January 8, 1892.

[2] "Du Langage et de la Musique" (French ed.), chap. xxii.; see also his "Studien über die Association der Vorstellungen."

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She was even unable to trace a circle, except as part of a man (the head).

In a paper presented at the London meeting of the International Congress for Experimental Psychology last August, I insisted that voluntary movements are possible in the child only after a great variety of motor "elements" have become available through great diffusion and mass in involuntary (imitative) reactions.[1] The above phenomenon, thus explained, serves to illustrate the broader position.

As there is no literature on this subject, the question of "tracery-imitation" has not even been put before, to my knowledge, I should be glad to have opinions upon it. It is evident that if one hold the other theory of the visual apprehension of figure, *i.e.* that it is given by sight apart from sensations, of eye-movement, he could still hold the explanation which I have offered above, by substituting for the series of eye-movement sensations, a, a^1, a^2, &c., a series of visual sensations, v, v^1, v^2, &c. J. MARK BALDWIN.

Difficulties of Pliocene Geology.

CONSIDERING the very great importance which the later tertiary beds must occupy in all speculations about the origin of man and the present geographical distribution of plants and animals, it is unfortunate that they should have attracted so little attention among English geologists.

The fact is perhaps not unnatural when we consider how very scantily they are represented in this country; the Norwich Crag being virtually the only bed where remains of pliocene land animals have occurred. The Norwich Crag is itself a very puzzling bed, where marine remains and land remains are found mixed together, the whole having been reassorted, and I do not know of a single pliocene land surface remaining intact in Britain. The so-called forest-bed can no longer be classed as pliocene, but is clearly of pleistocene age. A real mark of the true pliocene horizon in Europe is the occurrence of the mastodon and its associated fauna.

If we are to use the mastodon as a test we shall have to travel southward as far as Auvergne, if we are to find a pliocene land surface *in situ.* Unfortunately Auvergne is a very dislocated and broken country, and the sequence of the later deposits is very hard to make out, and I much question whether it be possible to find sections showing the true reading of the beds in question nearer than Florence.

I am writing in the hope that I may persuade Dr. Forsyth Major, who knows the valley of the Arno so well, to communicate to NATURE some account of the results arrived at by the Italian geologists.

At present the question is one of great perplexity. Let me refer to two points. First, How comes it that in no part of the world, so far as I know, has a single fragment of an undoubted pliocene beast been found in a cave? The carnivora of pliocene times must have frequented caves just as much as the bears and hyenas of pleistocene times, yet how comes it that we can nowhere find any tertiary remains in any cavern? It will not do to appeal to denudation, for if there be deposits anywhere protected from denudation it is those in caves. Can it be that every mountain chain where limestone rocks occur is younger than pliocene times?

Again, we know that in America, both north and south, the mastodon survived to the end of the Pleistocene age, and lived alongside of the mammoth and the Columbian elephant. In Europe there is very great doubt whether the mastodon and any form of elephant were ever contemporaries. No doubt the teeth of the mastodon have been found with those of the elephant in the Crags, but the Crags have been so rearranged that it is impossible to draw any safe conclusions from them. It is at all events extraordinary that, according to the French geologists, the two beasts have never been found together in France. I believe the same conclusion has been arrived at by the Italian geologists, but upon this point there is some uncertainty, and it would be very interesting to have the opinion of so competent an authority as Dr. Forsyth Major upon the point. It is one of importance, for upon it depends largely the question of whether there was a continuity in Europe between the pliocene and pleistocene land, or whether, as I am disposed to believe, there was a break between the two involving perhaps a violent revolution. There are other interesting questions involved in the issue I have raised, upon which you may possibly permit me to write on another occasion. Meanwhile the burden of my present letter is to point out how little we really know about the pliocene land, and how useful it would be to know more. HENRY H. HOWORTH.

The Athenæum Club, December 5.

Meteors.

A FINE meteoric shower was observed here on the night of November 23, from 7h. 30m. to 12h. 30m., when the observations were interrupted by cloud.[1] The meteors were evidently "Bielids," the radiant at 8.30 being near a point, R.A., 1h. 20m.; Dec., 40° 30′. The radiant, however, was not well defined, its area being at least 4° in diameter. For a single observer, in a position which commanded only about one-sixth of the visible hemisphere, the meteors numbered about six a minute, which would indicate at least seventy-five a minute for the entire sky, exhaustively observed.

At ten o'clock two observers, standing back to back in an open space, counted 104 meteors in five minutes; the position of the radiant being then, R.A., 1h. 30m.; Dec., 41° 30′—very near Upsilon Andromedæ. At this time the radiant seemed to be rather more definite than earlier, and several nearly stationary meteors determined the place with reasonable precision.

An hour later a similar count by the same two observers gave 100 meteors in four minutes and a half, and the radiant was determined at R.A., 1h. 40m., Dec., 40°. The rate of frequency continued about the same until the sky clouded up an hour later, and must, I think, be estimated as high as from 80 to 100 a minute for the whole number that might have been seen by a sufficient corps of observers. This would foot up from 24,000 to 30,000 for the five hours.

I am not quite certain whether the apparent change in the position of the radiant is, or is not, real; but a motion very similar in amount and direction is given by Denza in his observations of the meteoric shower of 1885 (see NATURE, vol. xxxiii. page 151).

Comet "*f*" (Holmes's) was about 10° west and 4° south of the mean radiant at R.A , 0h. 40m., Dec., 360° 45′. It was barely visible to the naked eye.

Most of the meteors were very small, not exceeding the fifth magnitude; but a few, perhaps one in ten, were above the second, and in the course of the night four were seen which rivalled or surpassed Jupiter. The brighter ones left bluish trains, which remained visible for three or four seconds. The smaller ones often came in "flights" of three or four together, and fully half the paths were more or less curved and wavy from the resistance of the air.

It is worthy of notice that the heliocentric longitude of the earth at the time of the shower was about 62°, instead of 65° which was the longitude of the descending node of Biela's orbit at the last appearance of the comet in 1852, and was the longitude of the earth at the time of the showers of 1872 and 1885. The fact suggests the inquiry whether perturbations since 1885 will fairly account for such a recession of the node.

It is obvious also that if the meteoric swarms encountered by the earth in 1872 and 1885 were really moving in the orbit of Biela's comet (which at its last appearance had a period of 6·6 years), then the swarm encountered the other night, just seven years later, must have been an entirely different one—unless indeed the perturbations since 1885 can account for a retardation of nearly five months.

Last night was for the most part overcast, but a watch of fifteen minutes through occasional openings in the clouds showed only one or two possible Bielids. Evidently the shower was not continuing with any intensity. C. A. YOUNG.

Princeton, N.J., U.S., November 25.

Comparative Sunshine.

AFTER explaining that by "sunshine" I intend that which *would* fall upon the earth if there were no atmospheric obstruction, one must first notice the very elementary truth that the amount of such sunshine at any assumed time and place is in proportion to the altitude of the sun at noon, and also the

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[1] An abstract of the paper is to be found in *Science*, November 18, 1892, and also in the Proceedings of the Congress.

[1] Our "Eastern standard time" is just five hours slower than Greenwich time.

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